

TRANSACTIONS OF THE AMERICAN NEUROLOGICAL ASSOCIATION.

FOURTEENTH ANNUAL REPORT.

Tuesday (First Day), Morning Session.

The American Neurological Association convened at Willard's Hotel, Washington, D. C., September 18th, 1888, and was called to order by the President, Dr. James J. Putnam, of Boston. The President then delivered the annual address.

ADDRESS OF THE PRESIDENT, DR. JAMES J. PUTNAM.

Gentlemen of the Neurological Association :

It is my pleasant duty to bid you all welcome to Washington, in the name of yourselves; and to declare our meeting to be formally opened.

The Council has done, and will do, everything in its power to reduce to a minimum the routine business, and to leave you free to attend to the scientific communications which we have come to hear.

The programme is a full one, and I shall detain you only a few moments from attacking it.

I wish to ask your attention, during the space that your courtesies allow to the presiding officer, to the consideration of the question, whether the time has not come when this Association might increase its efficiency by adopting some plan of co-operative work, to supplement and assist the individual work of its members.

I purposely avoid for the moment the term "collective investigation," because I do not mean to propose that we should necessarily adopt the exact method indicated by that name, but only to express my belief that, in a general way, we have reached such a point in our development,

that we can count on each other enough, personally and scientifically, to be able to look to each other for support during the progress of our undertakings, instead of only for criticism at their close.

The possible advantages to be expected from co-operation in some portions of our work are, I think, incontestible; the difficulty lies in realizing them.

That which usually gives to a scientific enterprise its pith and point, is the genius, faith, energy, or personal ambition of a single man, and I do not propose that we should lose sight of this fact. Nevertheless, I think we may make a step forward by attempting systematically what we now do in a desultory manner, in the way of mutual assistance.

No one of us, not even the most talented and industrious, is able to utilize for the purposes of original investigation more than a small proportion of the vast array of facts that crowd in upon him on his daily rounds; but we should be ready to devote ourselves with increased zeal to studying and classifying those that remained, if we were sure that our observations were being made in such a way that each one would lend its testimony to the proving or disproving of some particular hypothesis of pathology or physiology or therapeutics. For this purpose, however, uniform methods of examination and tabulation would be necessary, and the knowledge that some one stands ready to collaborate the material that has been accumulated, so that our time and labor should not have been thrown away.

Again, there are many subjects of the very highest practical importance, those namely, where the problems at stake are of a statistical character, which can only be dealt with satisfactorily through some species of co-operative inquiry. And co-operation, directed to this end, means simply an attempt to avoid the annoying differences and errors as regards method of examination and point of view, that come in to vitiate the statistics of observers working at different times and places and without mutual understanding.

I am speaking as if co-operative investigation were a new thing, whereas, as we all know, it has been tried, in

various forms, a hundred times, sometimes successfully, sometimes unsuccessfully, more often, no doubt, with partial and temporary success, then to be given up or changed for something better. Such temporary success even is often justification enough for the experiment. One does not look for institutions of millennial permanence, but for ever new steps in advance.

Let us glance at the history of one or two of the prominent co-operative efforts of recent times.

The first Collective Investigating Committee of the British Medical Association was appointed in 1881, with Dr. Mahomed as paid Secretary. Two volumes of records have been published, besides reports in the *British Medical Journal*, on various subjects.

At the Industrial Congress at Copenhagen in 1884, an address was made by Sir William Gull, favoring the appointment of an International Committee for a similar purpose, and the motion was warmly seconded and the committee appointed. The Verein für innere Medicin, in Berlin, had already given its sanction to this method of research, and had formed a committee with Leyden as chairman.

It could not be claimed, perhaps, that the work of these committees has always been of the first quality. The information was collected through circulars sent about rather broadcast, and the answers must have been of varying degrees of merit. There can be no doubt, however, that something substantial has been gained, not only of the nature of harvest, but also of seed-grain, since those who took part in the inquiry must have been trained and stimulated, so that another time they would both observe and report to better advantage.

An Association like ours would have a better chance of success in carrying out a plan of investigation of this kind than where the profession at large was called upon to give its co-operation; since most of our members are familiar with the methods of accurate research, and would understand the bearings of the investigation that we should make.

To speak of institutions nearer home, some of the New York physicians formed, a number of years ago, a society for collective investigation in therapeutics. It lasted, I believe, but for a few years, but during that time published papers of real value.

If care were taken to select questions for inquiry which were of fundamental and practical importance to every observer, such a scheme would have a greater prospect of permanence.

It is not, however, at the history of committees for collective investigation that I look with so much interest, as affording examples for our instruction, as to the work of individuals whose influence and energy have created schools and inspired followers; men like Prof. Charcot, to name but a single instance.

I have also in mind the results that have been gained, especially in the department of physiology, by dividing up an important inquiry into a number of related parts, which are assigned to different persons acting temporarily as assistants to one person, by whom the investigation is mainly undertaken.

Charcot's are not to be had for the asking; but we have our share of workers with ability, energy, and zeal enough to be good workers in given directions, and worthy of support. My own feeling is that there is more vitality in a scheme for which one or two persons make themselves responsible, especially if they are already identified with it, than in the work of a committee, where the interest is less personal and the responsibility more divided; and that the efforts of our Association should be directed to furthering the plans of such persons, by moral and scientific support and by furnishing funds for printing circulars if necessary, and so forth.

I can see no better way of accomplishing this than that the proposer of any investigation should appeal, either personally or through the Secretary, and, if necessary, at the expense of the Association, to the members, stating his plan and asking for support. If his scheme recommended itself to others so far that he should be able to form a suffi-

ciently large working committee, he should confer again with them and submit details. The rest of the committee should have the right of criticism, and of withdrawal if not satisfied, and the publication of the results should be made in the name of the committee with the originator as chairman; or, if it was agreed that the originator should do the greater part of the work, and make himself responsible for the conclusions, he might publish the results in his own name with those of the rest as collaborators; and the question as to whether the Association should vote an appropriation for his assistance, if any was required, and the amount of the appropriation, should be decided in any way that the members of the council might determine.

It is not my purpose to discuss in detail what subjects would be best suited for investigation in this manner, and I will refer only to one or two by way of illustration.

In the first place, there are those such as were proposed for the British Medical Society, including the vast subject of heredity; the laws of degeneration and (as being of quite equal importance) of regeneration in families and races; and the strange transmutation or evolution of diseases from one form to another, which seems to be entirely distinct and different.

To come nearer home, there are various questions in therapeutics, such as the action of iodide of potash on non-syphilitic brain tumors, as one member suggested to me.

Again, we all know that Dr. Dana has been studying the racial relations of the neuroses, a subject which Clifford Allbutt also has recently touched upon in his suggestive address, and for the investigation of which no country could be better suited than ours. It would be an easy matter for us to collect large numbers of facts under his guidance upon these points, and the comparison of the experience of the different cities would be particularly instructive.

The subject of cranial measurements, for which large masses of facts are also needed, is another fit matter for such methods of research, and I am glad to be able to say that a valuable introduction to investigation of this kind is among the candidates' papers for this year.

I have myself been interested for a number of years in an inquiry which I think could soon be brought to a fairly definite conclusion in this manner, namely, the effect of very small doses of lead as predisposing to disease. I published, last year, the report of the analysis for lead of the urine of eighty-six persons with various symptoms of nervous disease, functional and organic, but not presenting unequivocal symptoms of lead poisoning. In forty-eight cases, or more than fifty per cent. of the whole number, lead was found, and for certain groups of cases the proportion ran up to a much higher figure. Out of a group of eleven cases, on the other hand, made up of persons who were practically in good health, lead was found in only two cases.

These facts seemed to me of importance ; but it was also evident that their clinical significance would be diminished if it should appear, on further investigation, that the urine of most or of many healthy persons also contained lead, showing that its presence need not be injurious. I therefore made arrangements to have the urine of a large number of medical students analyzed, and up to the present time I have reports on twenty-three cases. In only three of these, or thirteen per cent., lead was found, and then in very small amounts.

Even these few affirmative cases, together with others of a similar kind which I have collected but which do not belong to this particular set, show that the mere finding of lead in the urine is not an indication of active poisoning ; but, on the other hand, it remains to be proved that the majority of persons can carry lead about with them all their lives and still keep their tissues as healthy and as resistant as if it were absent ; and it is still an important and open question, and one that only the testimony of large numbers of facts can answer, whether the first signs of poisoning are always the classical symptoms with which we have long been familiar, or whether lead, like syphilis, may predispose the central nervous system to degenerative changes of an entirely different order from those seen in typical cases of poisoning.

Such an investigation is too expensive, and requires too

much material, for a single person to deal with satisfactorily; but if a large number were to take part, reasonable conclusions could soon be reached; and it is certainly important for us to understand accurately the effect of influences to which the whole community is more or less exposed.

But I do not think it is by collective investigation alone that the members of the Association can be useful to each other, and to the profession, through co-operative work.

In the first place, I hope that another year we may do still more than we have done this year in the way of notifying members of the contents of papers to be read. The Surgical Association has, I think, set us a good example in printing quite a summary of the points to be raised in their discussions, and I feel sure that others besides the principal speakers will be stimulated thereby to take an active part.

Another piece of joint work which has occurred to me is the formation, either actually or on paper, of a collection of microscopic or other specimens, to be kept by the secretary or by an officer appointed for that purpose, or deposited in the Army Nat. Museum. If the collection should consist of actual specimens, they would naturally be duplicates; if it should consist of a list only, this might contain a statement of the preparations which members were willing to lend, under suitable conditions, and of those which they would only allow to be consulted in their own laboratories.

Perhaps this scheme is chimerical and would lead to nothing. If the specimens were likely to be largely consulted, I admit that their owners might be put to inconvenience. As a matter of fact, however, I presume they would be consulted but little, but I think that occasionally this would be done, and with the greatest possible benefit.

If this plan should lead to the formation of a central museum of such specimens as crania and brains which had been described, and would otherwise lie idle on a top shelf, or of photographs, drawings and casts of such specimens, the plan would, I think, be justified. Possibly we might some day publish sets of illustrations on the plan of the new *Iconographie of the Salpêtrière*.

[The National Army Museum, as Dr. Billings assured us in his address, is ready to take charge of microscopic and other collections, and what could be a more pleasant and fitting memorial of our Association than to have its name linked with such a collection, and to have been the first to set an example which would be sure to find followers.]

I have intended, in these remarks, only to ask you to reflect whether some way cannot be found through which, by combining, we can reach certain ends either of the nature of actual discovery, or of critical suggestion, which could not be reached by one person alone; and I have tried to indicate certain methods by which we should be the most likely to arrive at the best, though perhaps not the most conspicuous results, with the least danger of failing through overreaching ourselves.

If the general suggestion pleases you, I dare say better means than mine can be suggested for carrying it out. The time and place seem to me appropriate for initiating such an enterprise. This is the first meeting of the Congress of all the principal Associations of the country for their mutual stimulation and support, and our first meeting in the capital of the country.

What is, however, of more importance, we have, I think, reached a period in our development, when such a step is justifiable.

We have learned to know each other better personally, and we have all of us learned to do at least fairly good work, and some of us work of the highest quality, and all of us have learned to appreciate in what really good work consists.

A remark made to me ten years ago by one of the most prominent neurologists of Germany, that he could not make much use of American cases, they were so inadequately reported, would now not be in place.

We need now to bring our best workers more and more prominently to the front, and give them a chance to exert all the influence of which they are capable; to show that we appreciate that our specialty is bound to be of more

vital importance than almost any other, and that it is, above all, the specialty of research.

And it should be remembered, also, that we are not a local, but a national Association, with natural functions of the nature of encouraging broad and liberal enterprises of education and investigation, and not merely a collection of workers, bent only on prosecuting our individual aims, and with no bond of union but our yearly meetings.

REPORT OF THE TREASURER.

Dr. Graeme M. Hammond, of New York, Secretary and Treasurer, presented the Annual Report, which, on motion, was accepted. There was a balance in the treasury of \$217.60.

SCIENTIFIC COMMUNICATIONS.

Dr. Philip C. Knapp, of Boston, read a paper on Nervous Affections following Brain Injury ("Concussion of the Spine," "Railway Spine," and "Railway Brain").

(For paper see October number.

Dr. ISAAC OTT read a paper entitled

HEAT CENTRES IN MAN.

In this abstract I will give only the principal points of my paper on this subject.

There have been localized in the lower animals six centres whose injury causes increased temperature. The cruciate about the Rolandic fissure, the sylvian at the junction of the supra, and post-sylvian fissure; the caudate nucleus, the tissues about the corpus striatum, a point between the optic thalamus near the median line; and the antero inner end of the optic thalamus. These centres have their own laws, which serve to distinguish one from the other. Dr. W. Hale White¹ has published a paper containing a number of collected cases bearing on this subject, and I have mainly used them to support the theory of localized heat-centres in man. He has ruled out all cases where the cause is not purely nervous.

In support of a heat-centre about the Rolandic fissure,

¹ Guy's Hospital Reports, 1884.